their squares and products may be neglected, each of the residual coefficients may be represented by a linear function of the errors of the assumed coefficients, and the formation of the corresponding linear equations constitutes the second operation in Sir George Airy's method. The solution of these linear equations by successive approximations will finally give the corrections which must be applied to Delaunay's coefficients in order to satisfy the differential equations.

Now, since the proportionate errors of Delaunay's coefficients of parallax are considerable, and much greater than the errors affecting his coefficients of longitude and latitude, it will be readily understood that the result of the substitutions will be to leave considerable residual coefficients in the two equations which relate to motion parallel to the ecliptic, and much smaller residual coefficients in the third equation which relates to motion normal to the ecliptic, since in this last equation every error in the coefficients of the radius vector or of its reciprocal will be multiplied by the sine of the inclination of the Moon's orbit. This result, which might thus have been anticipated, is exactly what Sir George Airy has found to take place, according to a memorandum which he has recently addressed to the Board of Visitors of the Royal Observatory.

Since the errors affecting Delaunay's coefficients of parallax are comparatively large, it will be necessary to determine the factors by which these errors are multiplied in the equations of condition with a much greater degree of accuracy than is required in the case of the factors by which the errors of the coefficients of longitude and latitude are multiplied in the same equations. Otherwise, it will not be possible to deduce these last-mentioned errors from the equations with the requisite degree of precision. It will be necessary to take special precautions in order to determine with accuracy the corrections of the assumed coefficients in the inequalities of longitude which have long periods.

On the Change in the Adopted Length of the "Tabular Mean Solar Day," which takes place with every Change in the Adopted Value of the Sun's Mean Sidereal Motion. By E. J. Stone, M.A., F.R.S.

The view which I have taken that the so-called "mean solar day" of any of our tables is not necessarily the true "mean solar day," and that the "mean solar day" in use before 1864, for the calculation of our *Nautical Almanac*, is most certainly not the same interval of absolute time as that now adopted, and called a "mean solar day," is without doubt a startling assertion, and one likely to be sharply criticised; but I did not make it without a most careful consideration of the subject, and I feel perfectly certain of its truth. After all, the statement is not more startling than the facts for which it accounts. The meri-

dians of all our observations have either run away from the Sun and Moon in the last nineteen years by about 27^s, or astronomers have made a mistake in their count, as I have indicated, to this amount. Neither of these alternatives may appear probable, but the observations show that one of the alternatives must be accepted.

I consider that I have *mathematically* demonstrated my point, but my attention has been called to a letter by Sir G. B. Airy, in the *Observatory* magazine for May, in which that distinguished astronomer expresses, in very polite, but clear terms, his dissent from my views. The subject which I have opened out is of such importance, and the consequences which have already resulted from the changes made in 1864 are so serious in character, that I think it my duty to point out in the clearest possible manner the nature of the mistake made, and the fallacy of the reasoning by which Sir G. B. Airy has supported his dissent from my views.

In all scientific discussions the great difficulty is to fix the exact point at which our views diverge. When this can be done, the settlement of a mere mathematical question like the present should be comparatively easy. I feel, therefore, deeply indebted to Sir G. B. Airy for a clear statement of the evidence which he considers conclusive against the correctness of my views, and I shall be careful not to understate the force of the objections urged. I believe that the difficulty thus presented is the one which has chiefly influenced opinion against my views.

Sir G. B. Airy states that our observed Right Ascensions are referred to the adopted tabular places of our clock stars, and that no important changes in the tabular places of the clock stars were made with the change from Bessel's to Le Verrier's expressions for the sidereal time at mean noon in the *Nautical Almanac* of 1864. He points out that we determine the mean time corresponding to an observed Right Ascension of the Moon on the meridian from the formula that it is the equivalent in mean solar time of the sidereal interval (Observed R.A. — Tabular sidereal time at mean noon), and if we take from one of the Greenwich volumes an observed R.A. of the Moon—for instance, March 22, 1880, R.A. of centre = $9^h\ 10^m\ 59^s{\cdot}02$—and compute from this expression the Greenwich mean time, first with Bessel's value of the sidereal time at mean noon, and second with Le Verrier's value of the sidereal time at mean noon, the results will differ slightly, but by only the $\frac{1}{366}$ of the difference indicated by me. If, therefore, we compute from Hansen's tables the position of the Moon for these two separate times, the tabular R.A. will only differ by the $\frac{1}{366}$ part of the quantity indicated by me, and it is therefore quite impossible that the great errors at present existing between Hansen's tables and observations can be due to the use of the two different expressions for the tabular sidereal time. Now, I admit all the statements of fact made by Sir G. B. Airy, but I deny his conclusions. The method adopted for the computation

of Greenwich mean time is defective, and our count is wrong because our unit of time has been changed.

In questions of time there are two things to be taken into consideration—the count t, and the absolute length of the unit T, in terms of which the count is kept. If t_1 is the count with unit T_1, and t_2 the count for the same interval of absolute time with unit T_2, then we have

$$T_1 t_1 = T_2 t_2.$$

I have proved that the change from n' to $n' + \delta n'$ for the adopted value of the Sun's mean motion in the units of time T_1 and T_2 has established the relationship

$$T_2 = T_1\left(1 + \frac{\delta n'}{n'}\right).$$

It follows therefore, that if the count, as now conducted, differs only slightly from that formerly adopted, or $t_2 = t_1$, the error made by this false reckoning is

$$T_2 t_2 - T_2 t_1 = -\text{time} \times \frac{\delta n'}{n'}.$$

This indicates that the error in our present reckoning in time is increasing at about the rate of $1^s{\cdot}46$ per annum. This correction, when introduced into our theories, accounts for the large discordances shown between theory and observation in the case of the Moon and also the Sun. But, of course, if Sir G. B. Airy's count is right, or t_1 *does* equal t_2 sensibly, then T_2 and T_1 cannot differ in the way indicated by me, and there must be *some* flaw in the reasoning, or some false assumption, in what I have given as a mathematical proof of the relationship

$$T_2 = T_1\left(1 + \frac{\delta n'}{n'}\right).$$

I have stated the case against my views as strongly and clearly as possible.

It might be sufficient for me to ask those who refuse to accept my explanation of the cause of the existing errors in our theories, *which are proved to exist by our observations*, to distinctly point out my errors of assumption or of reasoning. But I am prepared to take the more direct course of showing that the present count is erroneous, and that when rightly conducted t_2 is not sensibly equal to t_1.

The so-called mean time, which is directly computed from observation, is nothing more than the equivalent in mean solar time of the sidereal interval. Observed R.A. on meridian — tabular R.A. of meridian at mean noon, and it is the error made in computing this small interval on any particular day which Sir G. B. Airy considers the total error in count arising from the change from Bessel's to Le Verrier's expressions for the tabular R.A. of the meridian, and this interval, added to the count for "days"—

and here comes in the question of what " days " if the unit has been changed—is considered the Greenwich mean time deduced from observation. But an inspection of the form Observed R.A. on meridian—Tabular R.A. of meridian at mean noon, shows that the mean noon, in this way of calculating mean time, is the instant when Observed R.A. on meridian = Tabular R.A. of meridian at mean noon; and if we adopt two different expressions for Tabular R.A. of meridian at mean noon, we must have two distinct noons. The mean noon, according to Bessel's expression, comes earlier than the mean noon according to Le Verrier's expression on any particular day by the exact interval which Sir G. B. Airy supposes to be the total error which arises from the change from Bessel's expression to Le Verrier's; the accumulative effects of this error day by day in separating the tabular position of the meridian from the true position, are entirely neglected; and it is the neglect of this accumulation of errors in the count which has made Sir G. B. Airy's count wrong. Sir G. B. Airy has obtained nearly the same count in both cases because he has taken the count of so-called days in each case from the motion of the observed meridian with respect to the stars without considering that if the increase of R.A. in Bessel's day was 360°, it will no longer be 360° when a day of Le Verrier's scale is used, but a larger quantity than 360° in the proportion of

$$1 + \frac{\delta n'}{n'} : 1.$$

The following is a direct mathematical proof of the error made, but to avoid complexity I shall, at first, neglect any differences in the small terms multiplied by t^2. The method of treatment, however, is perfectly general.

Let

$$S = C + (n + a \cos \omega_0 - u)t + \lambda t^2,$$

and

$$L = C + (n' + p)t + st^2$$

be the true tabular sidereal times for the meridian of Paris and the mean longitude of the Sun when $365{\cdot}25 \times$ (the true physical mean solar day) is adopted as the unit of time. Then

$$n + a \cos \omega_0 - u - n' - p = 365{\cdot}25 \times 2\pi,$$

$$\therefore\ (1 + x)(n + a \cos \omega_0 - u - n' - p) = 365{\cdot}25 \times 2\pi(1 + x).$$

Let

$$(1 + x)n = N;\ (1 + x)a = A;\ (1 + x)n' = N', \text{ etc., etc.}$$

$$\therefore\ N + A \cos \omega_0 - U - N' - P = 365{\cdot}25 \times 2\pi(1 + x).$$

Then

$$S = C + (N + A \cos \omega_0 - U)\frac{t}{1 + x} + \lambda(1 + x)^2.\left(\frac{t}{1 + x}\right)^2$$

$$L = C + (N' + P).\frac{t}{1 + x} + s(1 + x)^2.\left(\frac{t}{1 + x}\right)^2.$$

Let

$$t' = \frac{t}{1+x}.$$

$$S = C + (N + A\cos\omega_0 - U)t' + \lambda(1+x)^2.(t')^2$$

$$L = C + (N' + P)t' + s(1+x)^2.(t')^2$$

$$\therefore\ S - L = 365{\cdot}25 \times 2\pi(1+x)t' + (\lambda - s)(1+x)^2.(t')^2$$

$$\therefore\ S = C + (N'+P)t' + 365{\cdot}25 \times 2\pi(1+x)t' + \lambda(1+x)^2.(t')^2$$

Consequently, therefore, the sidereal time of the meridian can never assume the form

$$S = C + At + 365{\cdot}25 \times 2\pi\,.\,t + \lambda t^2,$$

unless the physical day is known and the true values of n' and p also known.

But let

$$S' = C + (n' + \delta n' + p)t + 365{\cdot}25 \times 2\pi t + \lambda t^2$$

be the expression adopted by Le Verrier and now used in the *Nautical Almanac*, and suppose Bessel's value to have been the true value, I am really only concerned here with the change.

Then let

$$N' + P = n' + \delta n' + p,$$

which requires

$$(1+x)(n'+p) = n' + p + \delta n'$$

$$\text{or } x = \frac{\delta n'}{n'+p}.$$

Then

$$\text{true } S = C + (n' + \delta n' + p).t' + 365{\cdot}25 \times 2\pi\left(1 + \frac{\delta n'}{n'+p}\right)t' + \lambda t_{\prime}^2,$$

$$\text{tabular } S' = C + (n' + \delta n' + p)t + 365{\cdot}25 \times 2\pi t + \lambda t^2.$$

It is absolutely necessary for the *true* computation of mean time by the formula adopted that the theoretical expression for the R.A. of the meridian should be identical with the true expression.

A comparison of these expressions shows that in order to make S′ agree with S we must take

$$t = t' = \text{true } \frac{t}{1 + \dfrac{\delta n'}{n'+p}}$$

and introduce a term

$$365{\cdot}25 \times 2\pi\frac{\delta n'}{n'+p}.t'.$$

This term does not vanish at tabular mean noon, and *it is the neglect of this term* which makes Sir G. B. Airy's count wrong. This is a strict mathematical proof, which, without directly

assuming the change of unit, proves that it must practically have taken place with the adoption of the change from n' to $n'+\delta n'$ for the Sun's mean motion.

The error made has been that n' has been changed into $n'+\delta n'$ without recognising the necessity of the change of units, and the neglect therefore of the correction $365\cdot25\times2\pi\frac{\delta n'}{n'+p}\cdot t$ to the tabular R.A. of mean noon when derived from Le Verrier's tables.

If we neglect this term we preserve nearly the same count, because that depends chiefly upon the term $2\pi . t$, which is assumed the same in both Bessel and Le Verrier. The neglect of this quantity by preserving our count without preserving the same unit of time necessarily makes the absolute times all wrong. The error has been that the change of unit has not been recognised. No correction for the term $365\cdot25\ 2\pi\frac{\delta n'}{n'}$ has therefore been introduced into our count.

The following is the more general investigation. Since

$$(n+a\cos\omega_0-u-n'-p)=365\cdot25\times2\pi$$

we have

$$(1+x+y\tau)(n+a\cos\omega_0-u-n'-p)=365\cdot25\times2\pi(1+x+y\tau).$$

Let

$$n(1+x+y\tau)=\mathrm{N}\,;\ a(1+x+y\tau)=\mathrm{A};\ u(1+x+y\tau)=\mathrm{U}\,;$$
$$n'(1+x+y\tau)=\mathrm{N}';\ p(1+x+y\tau)=\mathrm{P}$$
$$\mathrm{N}+\mathrm{A}\cos\omega_0-\mathrm{U}-\mathrm{N}'-\mathrm{P}=365\cdot25\times2\pi(1+x+y\tau),$$

since

$$\mathrm{S}=\mathrm{C}+(n+a\cos\omega_0-u).t+\lambda t^2,$$
$$\mathrm{L}=\mathrm{C}+(n'+p)t+st^2$$
$$\mathrm{S}=\mathrm{C}+(\mathrm{N}+\mathrm{A}\cos\omega_0-\mathrm{U})\frac{t}{1+x+y\tau}+\lambda(1+x+y\tau)^2\left(\frac{t}{1+x+y\tau}\right)^2$$
$$\mathrm{L}=\mathrm{C}+(\mathrm{N}'+\mathrm{P})\frac{t}{1+x+y\tau}+s(1+x+y\tau)^2\left(\frac{t}{1+x+y\tau}\right)^2.$$

Let

$$t=\tau(1+x+y\tau).$$

Then

$$\mathrm{S}=\mathrm{C}+(\mathrm{N}+\mathrm{A}\cos\omega_0-\mathrm{U})\tau+\lambda(1+x+y\tau)^2.\tau^2$$
$$\mathrm{L}=\mathrm{C}+(\mathrm{N}'+\mathrm{P})\tau+s(1+x+y\tau)^2.\tau^2\,;$$
$$\therefore\ \mathrm{S}=\mathrm{C}+(\mathrm{N}'+\mathrm{P})\tau+365\cdot25\times2\pi\tau(1+x+y\tau)+\lambda(1+x+y\tau)^2.\tau^2.$$

Now consider a tabular expression

$$\mathrm{S}'=\mathrm{C}+(n'+\delta n'+p)t+365\cdot25\times2\pi.t+(\lambda+\delta\lambda)t^2.$$

Let

$$(N'+P)+\lambda\tau = n'+\delta n'+p+(\lambda+\delta\lambda)\tau$$

be made an identity by a proper determination of x and y: then since

$$N'+P=(n'+p)\,(1+x+y\tau),$$

we have

$$x=\frac{\delta n'}{n'+p} \quad \text{and} \quad y=\frac{\delta\lambda}{n'+p};$$

$$\therefore\ S=C+(n'+\delta n'+p)\tau+365\cdot25\times2\pi\tau\left(1+\frac{\delta n'.\tau+\delta\lambda.\tau^2}{n'+p}\right)+(\lambda+\delta\lambda)\tau^2.$$

Comparing this with the tabular expression

$$S'=C+(n'+\delta n'+p)t+365\cdot25\times2\pi.t+(\lambda+\delta\lambda)t^2,$$

we see at once that S′ can be made identical with S only by the following assumptions:—

(1) The tabular t is really τ.
(2) That we apply a correction

$$=365\cdot25\times2\pi\left(\frac{\delta n'.\tau+\delta\lambda.\tau^2}{n'+p}\right).$$

The conclusions to be drawn are the same as before. The tabular sidereal times at mean noon require the correction

$$365\cdot25\times2\pi\left(\frac{\delta n'.\tau+\delta\lambda.\tau^2}{n'+p}\right)$$

where τ may be taken as sensibly equal to t. This demonstration proves the necessity for a correction which if neglected would produce an apparent secular acceleration of 4″ in the Moon's mean motion.

The *rationale* of this method is simply that with every change in the unit of time we must, if we determine our count of days directly from the increase of Observed R.A. by 360°, introduce a correction to render this assumption true. If this is not done, we must of necessity make our theoretical Right Ascensions run away from our observed ones. There is no escape from such a false assumption.

Note on Mr. Stone's Paper in the last number of the "Monthly Notices."

The statement on page 343, line 8, and three following lines, is badly expressed. It should read: "From which it follows that the absolute time in an interval of t years expressed in terms of the unit t_1 exceeds the absolute time in an interval of t years expressed in terms of t_2 by

$$\frac{\delta n'}{n'}.t."$$